THE
MATHEMATICAL GAZETTE.

ALICE THROUGH THE (CONVEX) LOOKING GLASS.

The Presidential Address to the London Branch of the Mathematical Association, February 9th, 1918.

BY WM. GARNETT.

(Continued from p. 252.)

From the results of the experiments to which reference has been made, and of others, the mathematical Theory of Relativity assumes :

1. That the unaccelerated motion of a system cannot be detected by observations made on that system alone with standards belonging to the system.

2. The velocity of light in free space measured on an unaccelerated system is independent of the unaccelerated velocity of the source of light, and

3. Is independent of the velocity of the system.

4. Two observers moving with uniform relative velocity will agree in their measurement of that velocity, and

5. In the measurement of a line perpendicular to the direction of their relative motion.

It also assumes

6. The conservation of momentum.
7. The conservation of energy.
8. The conservation of electricity.

From these assumptions it is deduced that *

(1) The velocity of light in free space measured on an unaccelerated system in units belonging to that system is independent of the direction in which the system moves, and

(2) Independent of the velocity of the system.

(3) If two systems move with a relative velocity v, and c is the velocity of light in free space, to an observer on each system his own time unit appears to be less than the corresponding time unit on the other system in the ratio

of $\sqrt{1 - \dfrac{v^2}{c^2}} : 1$.

---

* See Carmichael's *Theory of Relativity* for more precise statements.

M

(4) Under the same circumstances, to each observer his own unit of length along the line of relative motion appears to be less than that of the corresponding unit of length on the other system in the ratio of $\sqrt{1 - \dfrac{v^2}{c^2}} : 1$.

(5) If two systems are moving with relative velocity v and there are two clocks in one system at a distance d apart in the direction of the relative motion which appear to an observer on that system to indicate the same time, to an observer on the other system the forward clock appears to be slow as compared with the other by $\dfrac{v}{c^2} \cdot \dfrac{d}{\sqrt{1 - \dfrac{v^2}{c^2}}}$. (This is identical with the expression $\dfrac{v}{c} \cdot \dfrac{x}{\sqrt{c^2 - v^2}}$ given above.)

(6) If two velocities in any directions, each of which is less than the velocity of light, are combined, the resultant velocity is less than the velocity of light.

(7) If a velocity equal to the velocity of light is compounded with any other velocity equal to or less than it, the resultant is equal to the velocity of light.

(8) The velocity of a material system can approach to, but can never reach the velocity of light.

(9) The mass of a body moving relative to the observer as measured by him in a direction perpendicular to the line of motion (*i.e.* the transverse mass) is greater than its mass when at rest in the ratio of $1 : \sqrt{1 - \dfrac{v^2}{c^2}}$.

(10) Under the same circumstances the mass measured in the direction of motion (*i.e.* the longitudinal mass) is greater than the mass at rest in the ratio of $1 : \left(1 - \dfrac{v^2}{c^2}\right)^{\frac{3}{2}}$.

From the last two theorems it follows that as the velocity of a material body approaches the velocity of light its mass approaches infinity and a finite force can produce no acceleration in it. Compare Alice as she approaches the principal focus.

(11) If two observers are moving with relative velocity v, to each observer his own unit of velocity in the direction of motion appears to be equal to that of the other observer, but in a direction perpendicular to that of the relative motion it appears to be greater in the ratio of $1 : \sqrt{1 - \dfrac{v^2}{c^2}}$.

(12) Under similar circumstances, to each observer his own unit of acceleration in the direction of the relative motion appears to be greater than that of the other in the ratio of $1 : \sqrt{1 - \dfrac{v^2}{c^2}}$, and in a perpendicular direction in the ratio of $1 : 1 - \dfrac{v^2}{c^2}$.

(13) Under similar circumstances, the measure of a force on one system by an observer on the other system is reduced in the ratio of $1 - \dfrac{v^2}{c^2} : 1$ when the force is parallel to the relative motion and increased in the ratio of $1 : \sqrt{1 - \dfrac{v^2}{c^2}}$ when the direction of the force is perpendicular to the relative motion.

(14) The momentum is not in the direction of the velocity in a strained medium, except in the case of uniform pressure.

Hence, according to the Theory of Relativity, it appears that, to two observers moving relatively to one another, and each provided with the same standards of measurements, the units of time, length and mass belonging to each observer will appear to the other observer to differ from his own by a quantity of the order of the square of the ratio of the relative velocity of the observers to the velocity of light, while length and mass will vary according as the measurement is along or perpendicular to the direction of motion. Moreover, no body will be able to move with a velocity greater than that of light, and the mass of a body approaches infinity as the velocity approaches that limit.

I will ask you to consider whether Alice through the Convex Looking-Glass, with her variable standards of length, mass and energy, is living in a wonderland more wonderful than that which Einstein and the other mathematicians of Relativity would have us believe that we inhabit.

There is one point worth noticing in connection with the limit of velocity imposed by the Theory of Relativity. It has often been remarked that the history of the world is pictured in space by the luminous vibrations in spheres expanding with the velocity of light. If we could travel faster than light and then look back, we should see, if our vision were sufficiently acute, our own past history. The Theory of Relativity deprives us of any hope of doing this while we possess material bodies.

Returning to Alice, the following is a summary of the principal results we have obtained set forth for the purpose of comparison with the results of the Theory of Relativity :

(a) Alice's units of length, velocity and acceleration along and perpendicular to the axis vary as d^2 and d respectively.

(b) Alice's units of mass measured with respect to motion along and perpendicular to the axis vary inversely as d^2 and d respectively, and Alice's mass approaches infinity as she approaches the principal focus.

(c) Alice's units of momentum and force are invariable.

(d) Alice's units of energy measured along and perpendicular to the axis vary as d^2 and d respectively.

These results are easily remembered.

We have throughout supposed that Alice and her toys are always very small compared with the radius of the mirror. Otherwise her standard of length when placed parallel to the axis would be more contracted at one end than at the other, and one side of her top would be more compressed than the other, so that points in the top would not truly describe ellipses. We have also supposed the relative velocity of the two Alices to be very small in comparison with the velocity of light, so that the internal and external examiners may have the same unit of time and agree as to the length of an examination. We have thus avoided any variation of our units due to the relative motion of the two Alices.

If Alice exerts pressure on her ball in a direction other than along or perpendicular to the axis of the mirror, it will appear to the external examiner that the force is not in the direction in which the ball begins to move, because to him the mass of the ball is different in the two principal directions ; in fact the ball will move along the semi-diameter of an ellipse, the axes of which are in the ratio of d to f, while the force appears to the external examiner to act along the corresponding radius of the auxiliary circle. Presumably Alice's only means of estimating the direction of a force will be by the direction of the motion produced, and if the force is exerted by a string the ball will follow the string, but it will appear to the external examiner that the pull is not in the direction of the string.

It is on account of change of motion taking place in a direction other than that of the force applied that physicists have concluded that the mass of a moving electron is different in the direction of its motion and in the transverse direction. With β rays it was possible to experiment with velocities

approaching the velocity of light. In Alice's land the momentum of a particle is not generally in the direction of its velocity. This is why the change of velocity is not generally in the direction of the force producing it.

It will perhaps occur to you that if the density of a piece of glass in Alice's hands is different in different directions the glass would produce double refraction in an oblique ray. This occurred to Lord Rayleigh in connection with the Theory of Relativity, the compression in the direction of motion being supposed to produce an effect, analogous to mechanical pressure, on a beam of light traversing it obliquely, but the result of the test was negative. Lorentz showed that the difference between the longitudinal and transverse masses of electrons precisely accounted for this result. Alice would probably find the same.

Similarly, it occurred to Professor Trouton that if the length and section of a wire were different, according as the length were parallel or perpendicular to the direction of the earth's motion, its electric conductivity would be different if it were turned from one direction to the other. In Alice's land, in turning the wire from the parallel to the perpendicular direction, the length would be increased $\frac{f}{d}$ times, and the sectional area diminished $\frac{f}{d}$ times, one diameter remaining unchanged. Hence, the resistance would be increased $\frac{f^2}{d^2}$ times, and this seems to offer to Alice a splendid chance of discovering the peculiarity of her space. Though in the Theory of Relativity the fraction $\frac{f}{d}$ had to be replaced by $\frac{c}{\sqrt{c^2 - v^2}}$, the delicacy of the measurement of electric resistance was such that a variation in the ratio of the square of this fraction would be readily detected and fairly accurately measured. Again, Trouton's result was negative, but again the result can be explained if the conductivity of the metal is due to the existence within it of free electrons, by the fact that the effective mass of the electrons is different in different directions, being greatest in the direction in which the length of the conductor is most contracted. Alice is not yet old enough to measure electrical resistance for herself. When she has gone to the Newnham and taken a course in the Cavendish Laboratory of Looking-Glass Land, she may try Professor Trouton's experiment, and will probably arrive at the same result, which will be explained in the same manner, as we have already seen how mass increases as length diminishes.

In connection with her top, reference has been made to Alice's measurement of angles, and the appearance of her protractor to the external examiner. Let us now glance at Alice's theory of parallels. Let Alice cut

FIG. 2.

out a rectangle, $ABCD$, from some suitable thin material, in a plane perpendicular to the axis of the mirror. The rectangle will appear as such to the external examiner as well as to Alice. Now let her place the rectangle so that DC is on the axis of the mirror and CB towards the principal focus. We will suppose that the length DC is comparable with d. Then to the external examiner the lengths of CB and $DA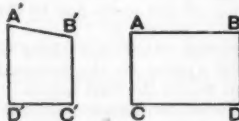$ will be proportional to their respective distances from F. Let $A'B'C'D'$ represent the figure which he sees. The side $B'C'$ will appear shorter than $D'A'$, and $A'B'$, $D'C'$ if produced will meet at F. To the external examiner, therefore, the angles at A' and D' will appear together less than two right angles. But to Alice the rectangle is her "set square," and all its angles are right angles in whatever position the rectangle is placed, so that according to her the angles at A' and D' are right angles, and according to Euclid's fifth postulate these lines will never meet, and according to Alice they never will, for the principal focus is her infinity, and

with whatever uniform finite velocity, measured by her standards, she approaches it she will never reach it.   To her, therefore, the straight lines $A'B'$, $D'C'$ never meet.

If we lived in Alice's world should we ever discover it ?   Even Euclid's fifth postulate, which is the discriminant between Elliptic, Euclidean and Hyperbolic space, fails to tell Alice that her space is not Euclidean.   No geometrical test appears to be of any avail, and as far as we have seen the principles of the Conservation of Momentum and the Conservation of Energy equally fail, while we have good reason to believe that optical tests and measurements of electrical resistance will follow the same course.

When I commenced the study of Euclid the parallel postulate was presented as the twelfth Axiom.   Had I known all that was involved in the acceptance or denial of this maxim I should have stood aghast on the threshold of Geometry.   Certainly my readiness to accept it would have been increased had I known what systems of geometry I should have to study if I adopted any alternative.   There are some mathematicians who contend that the geometrical studies of school children should not be confined to Euclidean space, and as we do not know in what kind of space we live, children should be taught geometry in its more generalised forms.   Perhaps it might be well to give them an insight into Elliptic and Hyperbolic Geometry as a warning of what they may expect if they repudiate Euclid's parallel postulate.

Before concluding, I may be allowed to say a word about the modern method of teaching the, so-called, practical mathematics.   We have long wanted methods more direct than those which were current fifty years ago for teaching useful mathematics to boys whose future professions demand the application of mathematics to practical problems.   Such methods have been provided by Professor Perry and others, whose work has been of very great value to young engineers, and in many cases has greatly influenced the teaching of mathematics, not only in Technical Schools, but in Secondary Schools also.   Nevertheless, I want to protest against mathematics being regarded merely as a tool to be used in order to obtain a result which is wanted for other purposes.   I might take you round a factory not far from here where you would see tool-minders feeding automatic machines.   The tool-minder is instructed, if the machine goes wrong, to cease work and await the arrival of the engineer.   The machine requires to be fed always with material in the same form, and its output should be invariable.   The machine-minder is much in the position of a boy who has been taught practical mathematics merely as a tool.   If there is a variation in the form of the problem presented, he is incapable of dealing with it, for he cannot adapt his machine.   The "practical mathematician" is in a worse position than the machine-minder, for frequently he cannot tell whether his machine is working accurately or not.   He has no means of testing the quality of the output.   If anyone is to be entrusted with responsibility for design, it is absolutely essential that he should have sufficient knowledge to enable him to test roughly the accuracy of his results by very simple calculations based on first principles.   Otherwise, if his machine goes wrong, its output may lead to very serious consequences.   There is a stage in the training of every mathematician, except, perhaps, a Cayley or a Sylvester, at which abstract mathematics should be applied to the problems of life, but there is also a stage at which abstractions should be studied for their own sake, and for the increased mental grip of practical problems which their study affords.   My own view is that the training of every pupil in mathematics should comprise three stages, though these stages overlap to an unlimited extent.   The stages may be styled familiarity, abstraction and application.   Some teachers go wrong by introducing abstractions, such as Euclid or rational mechanics, to children before they have been made familiar with the mundane realities of which these abstractions are the celestial types.   A child should be made

familiar through his senses with geometrical figures of three and two dimensions by toys, modelling, drawing, etc., before he is called upon to study the properties of the corresponding abstract and perfect figures. Similarly he should gain some acquaintance with the composition and resolution of forces and the principle of moments, by trying experiments before he proceeds to abstract reasoning about the conditions of equilibrium.

But while it is important that familiarity should precede abstraction, and that children should not be prevented from cutting their bread into cubes, prisms and rhombohedra at the nursery breakfast table, it is still more important that the stage of abstraction should not be omitted in the training of students who are to apply their mathematics to practical purposes. The extent to which abstract studies should be carried depends entirely on the level of the platform on which the student is to make his applications, and should always be in advance of the applications required. Familiarity, abstraction, application, correspond roughly to the work of the Elementary school, the Secondary school and the Technical school, but, wherever possible, the University should be included in the second and third stages.

We have recently introduced from America the term "vocational training," and there is some danger lest vocational training and technical training should be regarded as identical, but there is all the difference in the world between the application of mathematics and other branches of science to the understanding of workshop problems and that teaching of technique which properly constitutes vocational training.

Are we living behind a convex mirror? I have said that I do not think that from the results of experiments we can decisively answer this question. Those of you who accept the Theory of Relativity with all its conclusions, will, perhaps, be induced to apply your mathematics to the investigation of Alice's life problems to a higher order of accuracy than I have attempted in my hasty sketch of her adventures through the Convex Looking-Glass, and if this leads to a definite answer to my question, I shall need no apology for introducing Alice to a Mathematical Society.   WM. GARNETT.

GLEANINGS FAR AND NEAR.

25. *A.* I rose at daybreak, and after dressing, I wrapped myself in a blanket from my bed on account of the excessive cold—having no fire at that hour—and read algebra or the classics till breakfast time. I had, and still have, determined perseverance, but I soon found it was in vain to occupy my mind beyond a certain time. I grew tired and did more harm than good; so, if I met with a difficult point, for example, in algebra, instead of poring over it till I was bewildered, I left it, took my work or some amusing book, and resumed it when my mind was fresh. . . .—*Personal Recollections of Mary Somerville,** edited by her Daughter.

B. I had now read a good deal on the higher branches of mathematics and physical astronomy, but as I had never been taught, I was afraid that I might imagine that I understood the subjects when I really did not; so by Professor Wallace's advice I engaged his brother to read with me, and the book I chose to study with him was the *Mécanique Céleste*. Mr. John Wallace was a good mathematician, but I soon found that I understood the subject as well as he did. I was glad, however, to have taken this resolution, as it gave me confidence in myself, and consequently courage to persevere. . . . As soon as our engagement was known I received a most impertinent letter from one of his sisters, who was unmarried, and younger than I, saying, she "hoped I would give up my foolish manner of life and studies, and make a respectable and useful wife to her brother." I was extremely indignant. . . . —M. S.

* Hereinafter referred to as M. S.

THE INTRODUCTION TO INFINITE SERIES.*

By W. J. Dobbs, M.A.

(*Continued from p. 256.*)

(vi) *Binomial Series.* If it is desired to deal with the Binomial Series for any index before attacking the general case of Taylor's Series, the following method is suggested :

When a is a positive integer,

$$(1+x)^a = 1 + ax + \frac{a(a-1)}{\underline{2}} x^2 + \dots \dots \dots \dots \dots \dots \dots \dots \dots (3)$$

The series on the right terminates at the $(a+1)$th term. The meaning assigned to b^x, when b is positive and x is not a positive integer, is based upon the desire to render the complete graph of b^x a continuous curve ; the various extensions of the meaning of b^x as x becomes negative, zero and fractional result in the production of a graph which has the characteristic of being a continuous curve.† It becomes necessary to examine whether the statement (3) is limited to the case in which a is a positive integer. The removal of such limitation changes the series on the right into an infinite series, the general term, namely the $(n+1)$th, being

$$u_n = \frac{a(a-1)(a-2)\dots \text{ to } n \text{ factors}}{\underline{n}} x^n.$$

When a is changed into $a-1$, we will denote the corresponding $(n+1)$th term by v_n.

Thus

$$v_n = \frac{(a-1)(a-2)\dots \text{ to } n \text{ factors}}{\underline{n}} x^n.$$

It can be shown that, so long as a remains finite, both u_n and v_n can be made numerically as small as we please by sufficiently increasing n, provided x ranges in value between $-p$ and $+p$, where p is a positive quantity less than unity.

Let ϕ_n denote $1 + ax + \dots + \dfrac{a(a-1)\dots \text{ to } n \text{ factors}}{\underline{n}} x^n$,

i.e. $\phi_n = 1 + u_1 + u_2 + \dots + u_n.$

Differentiating with respect to x and multiplying by $\dfrac{1+x}{a}$, we obtain a result which differs from ϕ_n by v_n. This suggests that the limit of ϕ_n when $n \to \infty$ may be ϕ, such that $\dfrac{1+x}{a} \dfrac{d\phi}{dx} - \phi = 0$ and $\phi(0) = 1$. If so, ϕ is at once identified with $(1+x)^a$.

Examining this point more carefully, we have

$$\frac{1+x}{a} \frac{d\phi_n}{dx} - \phi_n = -v_n.$$

Multiplying by $a(1+x)^{-a-1}$, we have

$$\frac{d[(1+x)^{-a}\phi_n]}{dx} = -\frac{a}{(1+x)^{a+1}} v_n.$$

Now, v_n can be made numerically as small as we please by sufficiently increasing n, provided x ranges between $-p$ and $+p$; at the same time $\dfrac{a}{(1+x)^{a+1}}$ remains finite, though it varies with x. It appears then that $(1+x)^{-a}\phi_n \to$ a constant value throughout such a range. But the only possible constant value is unity, since $(1+x)^{-a}\phi_n = 1$ when $x = 0$.

---

* Read at a meeting of the London branch on the 9th of March, 1918.

† See *Mathematical Gazette*, vol. viii. p. 119.

We infer that $(1+x)^{-a}\phi_n \to 1$ as $n \to \infty$, and consequently, as $(1+x)^a$ is finite, $\phi_n \to (1+x)^a$ as $n \to \infty$ for all values of x ranging from $-p$ to $+p$. Thus $(1+x)^a$ is the limit of the sum

$$1 + ax + \frac{a(a-1)}{\underline{|2}} x^2 + \dots$$

so long as x ranges from $-p$ to $+p$, p being a positive quantity less than 1.

Also, if we desire an expression for the remainder after $(n+1)$ terms when a particular value h is assigned to x, we have

$$(1+h)^{-a}\phi_n(h) = 1 + h\left[-\frac{a}{(1+\theta h)^{a+1}} \right] v_n(\theta h),$$

whence $(1+h)^a = \phi_n(h) + R_m,$

where $$R_n = ha \cdot \frac{(1+h)^a}{(1+\theta h)^{a+1}} \cdot \frac{(a-1)(a-2)\dots \text{ to } n \text{ factors}}{\underline{|n}} \theta^n h^n.$$

(vii) *Taylor's Series.* All these series are, of course, but particular cases of Taylor's Series. It seems desirable to consider a few particular cases in order to prepare the way for the general theorem, but one should not unduly prolong the discussion of particular cases. Time does not permit me to describe in detail any method of approach to Taylor's Series. A very good one will be found in the new book by Professor Carey, to which reference has already been made. I proceed at once to place before you a method of attacking the proof of Taylor's Series, which may perhaps be found worthy of consideration from the school point of view. The classical proof giving one or more forms for the remainder is a hard nut for any schoolboy to crack, and I have known University students who have merely "got it up" without understanding it.

Let $f(x)$ be a function of x which is continuous while x ranges between certain finite values, and suppose that $f'(x), f''(x), f'''(x), \dots$ are also continuous for this range of x. We refer to this range as the range of continuity.
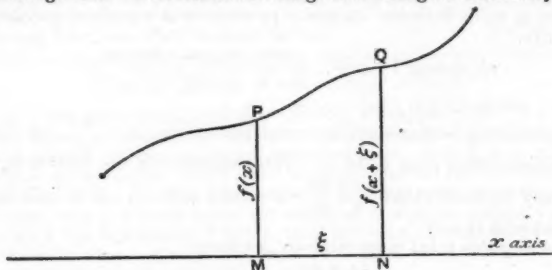

Fig. 7.

The figure represents the graph of $y = f(x)$ for values of x within the range of continuity. Any two values x and $x + \xi$ are taken within this range, PM and QN being the corresponding ordinates representing $f(x)$ and $f(x+\xi)$.

We have to consider the sequence of functions

$$\phi_1 = f(x) + \xi f'(x),$$
$$\phi_2 = f(x) + \xi f'(x) + \frac{\xi^2}{\underline{|2}} f''(x),$$

$$\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots\dots$$

$$\phi_n = f(x) + \xi f'(x) + \dots + \frac{\xi^n}{\underline{|n}} f^n(x),$$

each member of which involves two variables x and ξ.

Let $x+\xi=b$, so that $x=b-\xi$ and $\xi=b-x$, and let x and ξ vary while b remains fixed. Then the ϕ's may be expressed either as functions of x or as functions of ξ. Either method is effective. We will take the second alternative. It amounts to placing the origin at N and reversing the x-axis.

Thus $\phi_n=f(b-\xi)+\xi f'(b-\xi)+...+\dfrac{\xi^n}{\underline{n}}f^n(b-\xi),$

and each of the ϕ's becomes $f(b)$ when $\xi=0$.

Differentiating, we have $\dfrac{d\phi_n}{d\xi}=-\dfrac{\xi^n}{\underline{n}}f^{n+1}(b-\xi).$

Now $\dfrac{\xi^n}{\underline{n}}$ may be made as small as we please by sufficiently increasing n for all values of ξ within the range of continuity. It may happen that for all values of ξ ranging from 0 to p, not beyond the range of continuity, perhaps for a range restricted to a part only of the range of continuity, $f^{n+1}(b-\xi)$ remains finite, though this function varies with ξ, even when $n\to\infty$. In such cases $\dfrac{d\phi_n}{d\xi}$ can be made numerically less than ϵ by sufficiently increasing n for all values of ξ ranging from 0 to p. Under such circumstances $\phi_n\to$ a constant value as ξ ranges from 0 to p. But the only possible constant value is $f(b)$, since each of the ϕ's becomes $f(b)$ when $\xi=0$. If then our Lemma stands, the limit of the sequence is $f(b)$ for all values of ξ ranging from 0 to p, and we have here a proof of Taylor's Series for this narrow range of cases.

In other cases, though $f^n(b-\xi)$ may $\to\infty$ as $n\to\infty$, it may happen that $-\dfrac{\xi^n}{\underline{n}}f^{n+1}(b-\xi)$ may still be made numerically less than ϵ by sufficiently increasing n for all values of ξ within some limited range 0 to p. And in such cases also the limit of the sequence is $f(b)$ for all values of ξ ranging from 0 to p, i.e. $f(b)$ is the limit of the sum

$$f(b-\xi)+\xi f'(b-\xi)+\dfrac{\xi^2}{\underline{2}}f''(b-\xi)+....$$

The validity of the expansion is dependent upon the continuity of the various functions and the possibility of making

$$\left|-\dfrac{\xi^n}{\underline{n}}f^{n+1}(b-\xi)\right|<\epsilon \quad ....(4)$$

by sufficiently increasing n for all values of ξ ranging from 0 to p.

Further, if R_n denotes the remainder after $(n+1)$ terms for a particular value h of ξ between 0 and p, so that

$$f(b)=f(b-h)+hf'(b-h)+...+\dfrac{h^n}{\underline{n}}f^n(b-h)+R_n,$$

we have $R_n=-h\left[-\dfrac{\theta^n h^n}{\underline{n}}f^{n+1}(b-\theta h)\right]$

$$=\dfrac{\theta^n h^{n+1}}{\underline{n}}f^{n+1}(b-\theta h).$$

Putting $h=b-a$, we have

$$f(a+h)=f(a)+hf'(a)+...+\dfrac{h^n}{\underline{n}}f^n(a)+R_n,$$

where $R_n=\dfrac{\theta^n h^{n+1}}{\underline{n}}f^{n+1}[a+(1-\theta)h],$

or writing $1-\theta=\theta_1,$ $R_n=\dfrac{(1-\theta_1)^n h^{n+1}}{\underline{n}}f^{n+1}(a+\theta_1 h).$

This is Cauchy's form for the remainder. The condition that $|R_n|<\epsilon$ is precisely the condition (4) above, h being finite, and θ_1 being undefined but within the range 0 to 1.   W. J. DOBBS.

A LETTER FROM SIR WILLIAM ROWAN HAMILTON.*

OBSERVATORY, DUNSINK,
September 9th, 1856.

My dear Lloyd,

　　I returned to this place only a week ago, having lingered for a quiet fortnight in Cheltenham, after the business and amusement of the Association week; which with me extended from Monday the 11th, to Saturday the 16th of August, for I was asked to several dinners, after the general meetings had closed, and visited the College and other shew-places such as Lord Northwick's splendid Picture Gallery, during that time. Conceive me shut up and revelling for a fortnight in John Graves's Paradise of Books ! of which he has really an astonishingly extensive collection, especially in the curious and mathematical kinds. Such new works from the continent as he has picked up ! and such rare old ones too ! Besides works of Archimedes and Apollonius, which I had read before, he shewed me the original edition of Copernicus's book *De Revolutionibus*, etc., containing an *apologetic preface* by the Editor, which Graves had not observed; and which, while modestly putting forward the motion of the earth as an *hypothesis*, endeavours to deprecate, by anticipation, the displeasure not of the priests, but of the *philosophers* ! for no fear seems to have been as yet entertained of awakening the wrath of the Church; and indeed I believe that the work was dedicated to the Pope of the time, but am not quite sure of this.

　　To descend to more recent times—though on my way to them I lingered for a good while on a charming folio of the works of Wallis, written in part in English first, but afterwards translated into Latin, for the greater ease of the reader; and including a defence of the Sunday against the Saturday, which latter day has (I believe) still some advocates in Christendom as being the Sabbath of the Bible—I was induced to read some modern German publications, chiefly on the Theory of Numbers, which is a favourite study of John Graves, though I have very little attended to it. I scarcely knew, before I was with him lately, that theorems respecting *real integers* have been extended to imaginary integers, such as $3+7\sqrt{-1}$, under the name of *complex numbers* : and that in this extended view the number 2 (for instance) ceases to be prime, because it is $=$ the product $(1+\sqrt{-1})(1-\sqrt{-1})$; though 3 remains a prime number.

　　Graves pointed out to me that, in a future theory of INTEGER QUATERNIONS, NO REAL INTEGER WILL CONTINUE TO BE PRIME, because in quaternions

$$w^2+x^2+y^2+z^2=(w+ix+jy+kz)(w-ix-jy-kz),$$

and *every real and positive integer is* known to be *the sum of four square numbers*, 0 included. I delighted him by dashing off a solution of a problem which he had supposed would be found difficult : namely to find the greatest common measure of two proposed quaternions. He named at random $1+2i+3j+4k$, and $5+6i+7j+8k$; and I soon assigned (by a general process) $i-k$ as their greatest common measure, multiplied, it is understood, by ± 1 or $\pm i$ or $\pm j$, or $\pm k$. In his first rapture he exclaimed, "I see that quaternions will do everything ! " You will remember that these are *his* words, *not mine*.

　　(This sheet happens not to have his signature, William R. Hamilton. His signature is shewn in another letter.)

　　There is a footnote in the margin on Lord Northwick:

　　"I happened to be introduced to his Lordship who is now old; and he told me that he well remembered the English Sir William Hamilton in Naples, and shewed me a miniature of Lord Nelson, and a gem of *his* Lady Hamilton."

NOTES ON THE LIFE AND WORKS OF COLIN MACLAURIN.

By Charles Tweedie, M.A., Carnegie Fellow.

Since my article on Maclaurin appeared in the *Mathematical Gazette* in 1915, a number of additional facts regarding him have come to my knowledge, which ought to be in the possession of any historian of Maclaurin and his Times. For the sake of brevity these are given in the following detached notes.

(1) Appointment at Edinburgh University.

In Volume II. of Brewster's *Life of Newton* are printed Newton's letter to the Provost of Edinburgh, recommending Maclaurin for the Chair of Mathematics; Newton's letter to Maclaurin; and Maclaurin's reply.

(2) Maclaurin's Dispute with Campbell.

See (5) Maclaurin and Jas. Stirling.

(3) The *Memorial on Gauging*, formerly in the possession of Sir James Russell, has been bequeathed by him to the Library of Edinburgh University.

(4) Maclaurin and the Foundation of the Physical Society in 1737-8.

Two important letters from Maclaurin to his friend, Dr. Johnston, Professor of Medicine in Glasgow, are printed in the *Scots Magazine* for 1804. The first, dated June 9th, 1737, tells the story of the foundation of the Society in which Maclaurin took so prominent a part. He and Dr. Plummer, Professor of Chemistry in Edinburgh, were joint Secretaries. Maclaurin was much disconcerted by the refusal of Robert Simson to join after having taken steps to get him nominated. From the second letter we learn that Maclaurin read two communications before the Society in 1738, one being on the *Figure of the Earth*. It did not appear in printed form; but a paper, bearing this title, is among the MSS. preserved in Aberdeen.

(5) Maclaurin and James Stirling.

Perhaps the most interesting discovery I have made bears upon the intimacy that existed between these two Scottish scholars, which is now revealed for the first time. Through the courtesy of the Stirling Family, and of the Aberdeen University Library, I have obtained copies of no fewer than ten letters from Maclaurin to Stirling, and four letters from Stirling to Maclaurin.

Stirling had been a hot-headed Jacobite, at least in his younger days at Oxford, and the two men may have been in opposite political camps. But there can be no doubt of their long and close friendship, and of the mutual interest in their mathematical researches, particularly on *Attraction*, and on the *Figure of the Earth*, to which theories both made notable contributions. (See Todhunter's History of these subjects. Todhunter was wrong in suggesting Simpson's name for Stirling's, and right in his conjecture that Maclaurin got his results independently of Simpson.)

The earlier letters are taken up almost entirely with Maclaurin's dispute with George Campbell. It appears that it was Maclaurin's own suggestion to Stirling that something might be done to help Campbell, who had been, in a sense, a rival of his own for appointment in Edinburgh.

Campbell's interests were looked after in the Royal Society by Sir A. Cuming, a friend of Stirling and De Moivre, who had so remarkable a career later, becoming a chief of the Cherokees, and ultimately dying in poverty in London. A paper on Equations by Campbell was printed, Stirling reading it in proof to oblige Machin, the Sec. R.S. Maclaurin was much annoyed to find that it

forestalled a second paper of his own on the same subject, and he sent his own results to Folkes. The matter did not end here, for Campbell then made a public attack on Maclaurin in print, to which Maclaurin found himself compelled to reply. I have not seen either the attack or the reply. Maclaurin and Campbell both failed in their object, which was to prove Newton's Theorem regarding the number of imaginary roots of a given equation,—a theorem that had to wait for demonstration until 1864-6, when Sylvester in a series of papers established it and furnished a generalisation (*v.* Sylvester's *Collected Works*; or Todhunter's *Theory of Equations*).

Maclaurin asked for, and obtained, Stirling's help and criticisms, while his *Treatise of Fluxions* was in proof.

This had an interesting sequel. About 1736, Euler wrote to Stirling a letter (now lost) in which he communicated his *Summation Formula*. Stirling in his reply recognised its importance, and that it included his own theorem on log n ! as a particular case, but " warned " Euler that he had read the *identical* theorem in the *Fluxions*.

Euler in a long reply, full of interesting mathematical information, waived his claims to priority before Maclaurin.

On the whole, Reiff's suggestion (*Geschichte der Unendlichen Reihen*), to call the theorem the Euler-Maclaurin Summation Formula seems well justified.

I hope some day to see published the mathematical correspondence of Stirling, much of which is of great interest to mathematicians.

Maclaurin had also frequent correspondence with Clairaut, the classical authority, after Newton, on the Figure of the Earth.

(6) MACLAURIN AND THE REBELLION OF 1745.

A letter from Maclaurin to the Lord President Forbes is printed in the *Culloden Papers* (p. 262). It was written from the College of Edinburgh on 9th December, 1745, and gives an account of his movements immediately before and after the Rebellion. He tells how he travelled " from Morpeth to Waller on the bad Thursday, the 14th of November, and this day and the next two days got the most dangerous cold I ever had, from which I am not yet recovered."

I had the impression that he reached Edinburgh in a dying condition, but he was clearly able to perform his customary duties on his return. He died in June, 1746. I understand that his *Diary of the Siege* is in the possession of Dr. W. B. Blaikie, who is to publish it so soon as an opportunity arises. In his preparations of the defence of Edinburgh he was assisted by General Wightman (*Culloden Papers*, p. 224).

(7) MACLAURIN'S ESTATE.

After twelve years of vain search, I have ascertained that the statement, taken from the Introduction to the *Works of John Maclaurin of Dreghorn,* that he left an estate in Berwickshire, is not correct. The estate in question was Drygrange, situated on the Leader, about a mile above its junction with the Tweed. Drygrange forms part of the parish of Melrose in Roxburghshire, and was originally held by the monks of Melrose Abbey. In the Introduction to Vol. III. of the *Regality Records* of Melrose, by C. S. Romanes, C.A., the writer, in enumerating the various proprietors, makes the statement:

" Mr. Paterson sold them (*i.e. Drygrange*) to Colin Maclaurin, Professor of Mathematics in the University of Edinburgh, who in turn disponed them to Thomas Tod, W.S., who was born on 6th December, 1726, and died 26th December, 1800."

It was, however, not Colin, but his son John who disponed the estate, and purchased the property of Dreghorn in Colinton, near Edinburgh, from which, according to Scottish custom, he took the title of Lord Dreghorn when he became a Judge.

(8) Samuel Johnson's Contribution to the Epitaph of Maclaurin.

John Maclaurin and Boswell, the biographer of Johnson, were both members of the Scottish bar, and Maclaurin met Johnson on his visit to Edinburgh. The rest is best told as by Boswell (*Tour to the Hebrides*, Chap. II.):

" Mr. Maclaurin's learning and talents enabled him to do his part very well in Dr. Johnson's company. He produced two epitaphs upon his father, the celebrated mathematician. One was in English, of which Dr. Johnson did not change one word. In the other, which was in Latin, he made several alterations. In place of the very words of Virgil,

> *Ubi luctus et pavor, et plurima Mortis imago,*

he wrote

> *Ubi luctus regnant et pavor.*

He introduced the word *prorsus* into the line

> *Mortalibus prorsus non absit solatium ;*

and after

> *Hujus enim scripta evolve*

he added

> *Mentemque tantarum rerum capacem corpori caduco superstitem crede,*

which is quite applicable to Johnson himself."

Indeed, the last beautiful addition could hardly have been written by the son.

(9) Maclaurin and Goldsmith.

Professor Loria in a note to the *Gazette* referred to the " yawning " of Maclaurin. I have always considered that the fable concerned John Maclaurin, the son. Let me here record the facts.

In Goldsmith's *Animated Nature* (Vol. II. p. 91), published in 1774, occurs the following passage inserted by Goldsmith :

" Every one knows how very sympathetic this kind of languid motion is, and that for one person to yawn is sufficient to set all the rest of the company a-yawning. A ridiculous instance of this was commonly practised upon the famous Maclaurin, one of the professors at Edinburgh. He was very subject to have his jaw dislocated : so that when he opened his mouth wider than ordinary, or when he yawned he could not shut it again. In the midst of his harangue, therefore, if any of his pupils began to be tired of his lecture, he had only to gape or yawn, and the professor instantly caught the sympathetic affection : so that he thus continued to stand speechless, with his mouth wide open, till his servant, from the next room, was called in to set his jaw again." *

Boswell, in his *Life of Johnson*, narrates how John Maclaurin was indignant at this aspersion upon his father's memory, for which the law would give no reparation, Goldsmith being dead and buried ; and he adds a note to the effect that Nourse, the publisher, agreed to cut out the offending passage. But the later editions seem to indicate no alteration from the above with the footnote. Was the footnote the ultimate form of reparation, and was there an earlier edition of *The Animated Nature* (" since the publication ") ? Or was the footnote worked into the page of the first edition ? If so, should earlier copies without the footnote not be in existence ? Perhaps some scholar of English Literature can reply.

Of course, Goldsmith had studied medicine at Edinburgh, and the anecdote resembles a silly student's story.   C. Tweedie.

---

* "Since the publication of this work the editor has been credibly informed that the professor had not the defect here mentioned."

THE EQUILIBRIUM OF JOINTED FRAMEWORKS.

By Prof. G. H. Bryan, F.R.S.

In teaching classes the methods of calculating the stresses in roof trusses, warren girders, and aeroplane frameworks, by the use of force diagrams or the method of sections, it is customary to confine the calculations to the effect of loads applied at the joints. In an actual framework, on the other hand, it is often the weights of the various members that are of most importance, and, in such cases, the methods in question appear to fail. I have never seen a treatment of heavy frameworks that quite satisfied me. Some writers appear to make a distinction between systems in which the bars are held together by pins which are distinct from them and those in which the joint is formed by a pin attached to and forming part of one bar passing through a hole in the other. It is stated that in the former case the weight W of a uniform bar may be replaced by weights $W/2$ applied to the pins, but the explanation never seems to me very clear, and the following method is the first thing on the subject that I have been able to understand clearly.

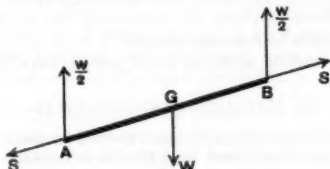Consider first the equilibrium of a single bar (Fig. 1). Let the reactions

Fig. 1.

at each end be resolved into a vertical component and a component acting along the bar. Then it is easy to show, either by resolving and taking moments or in a number of other different ways, that:

(1) The vertical components at the two ends are each equal to $W/2$ in the case of a uniform bar, or in other cases they divide the value of W in the inverse ratios of the segments into which the bar is divided by its centre of gravity.

(2) The component stresses along the bar at the two ends are equal and opposite, as denoted by S in the figure. The conditions of equilibrium, however, leave the stress S indeterminate, and it is to calculate this that a force diagram is necessary, as the value of S clearly depends on the effect of the other bars forming the framework.

Now the conditions of equilibrium will be unaffected if we replace the bar, if supposed uniform, by a light bar with weights $W/2$ attached to its end (*i.e.* applied to the *bar itself* and not to any pins), and if the bar is not uniform, its weight may similarly be replaced by suitable weights at the two ends with a light connecting rod between them.

This makes no difference in the reactions of the other members, and the stress S then becomes the tension or thrust in the light connecting bar.

The conclusion follows that the ordinary force-diagram method or method of sections *can* be used to calculate the stress component S in a bar if the weights of the bars are transferred to their ends.

The actual reaction on the bar at either end is the resultant of S acting along the bar and an upward force, which for a uniform bar is $W/2$.

It may easily be constructed graphically either in a separate diagram or even in the original force diagram.

It is now easy to calculate the stresses across any section of the beam (Fig. 2), and it will suffice to consider the case of a uniform beam or a uniformly
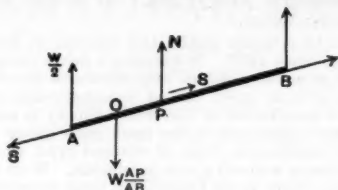

Fig. 2.

distributed load. In this case the reaction at P on the segment AP is compounded of :

(1) A stress S along the rod.

(2) A force N acting vertically upwards equal to

$$W\frac{AP}{AB} - \frac{W}{2} \text{ (positive if } AP > AG),$$

which is also equal to a downward force

$$W \cdot \frac{PG}{AB} \text{ (positive if } AP < AG).$$

(3) A couple representing the *bending* moment at P. If the inclination of the rod to the horizon is θ, we may, if we like, write this

$$= \frac{W}{2} AP \cos \theta - W\frac{AP}{AB} OP \cos \theta$$

$$= W\frac{AP \cdot PB}{2AB} \cos \theta,$$

or we may get rid of $\cos \theta$ by expressing it in terms of the projections of the several segments.

It will, of course, be open to any teacher to decide how much of this work he attempts to teach his pupils, but it does appear desirable that the teacher himself should have seen an explanation not less intelligible than the above, and the part which I have given above relating to the stresses and bending moment is inserted in order to *show how the thing works out*, rather than as implying that the brain of an average pupil should necessarily be worried with a bending-moment calculation at the end of a difficult roof-truss problem.

<div style="text-align:right">G. H. Bryan.</div>

---

25. *C.* We frequently went to see Mr. Babbage while he was making his Calculating machines. He had a transcendent intellect, unconquerable perseverance, an extensive knowledge on many subjects, besides being a first-rate mathematician. I always found him most amiable and patient in explaining the structure and use of the engines. The first he made could only perform arithmetical operations. Not satisfied with that, Mr. Babbage constructed an analytical engine, which could be arranged so as to perform all kinds of mathematical calculations and print each result.

Nothing has afforded me so convincing a proof of the unity of the Deity as these purely mental conceptions of numerical and mathematical science which have been by slow degrees vouchsafed to man, and are still granted in these latter times by the Differential Calculus, now superseded by the Higher Algebra, all of which must have existed in that sublimely omniscient Mind from eternity. . . . — M. S.

REVIEW.

Principes de Géométrie Analytique. Par Gaston Darboux. Pp. 519. 20 frs. 1917. (Gauthier-Villars.)

This volume gives to a wider public the content of lectures delivered in Paris at various times since 1872. It assumes a fairly complete acquaintance with the elements of geometry, but no very advanced knowledge.

Its chief aim is to give an exact idea of imaginary and infinite elements in geometry. The solid foundation of the whole theory is analytical, based on Cartesian coordinates. Throughout the book each group of ideas is linked with a special set of coordinates, some of unusual type, whose use makes the development of the theory natural if not inevitable. If an algebraic equation is made homogeneous, there is no longer any need to consider specially the reduction in degree when the leading coefficient vanishes ; so the use of homogeneous coordinates almost forces us to regard points at infinity as on an equal footing with finite points. A full discussion follows of tetrahedral coordinates, which leads on to cross ratio, homography, homology, and the principle of duality ; a chapter on anharmonic properties of a conic concludes Livre I.

In Livre II. the definition of angle and distance plunges us into the theory of isotropic lines, with a system of complex coordinates in two and three dimensions. The imaginary generators of a sphere are used to produce the usual formulae of spherical trigonometry. Livre III. deals with Poncelet's Theorems, with which is associated a system of coordinates based on a parametric form of the tangents to a conic. In Livre IV. Euclidean space is left behind for what Darboux calls Cayleyan geometry, in which the absolute is an arbitrary quadric, and distance is defined as the logarithm of a cross ratio ; there follows the appropriate trigonometry and theory of displacements. Livre V. treats of inversion ; the coordinates are pentaspherical, and the application is to cyclides, to which the last five chapters are devoted.

Geometry is treated as a whole, and not divided into watertight compartments ; pure and analytical methods, in the plane and in space, are used in turn or at once, as occasion offers. Though the justification of imaginary elements is insisted upon, due account is always taken of the reality of the figures under consideration.

Much of the charm of the book arises from the connections which are shown to exist between ideas, familiar and unfamiliar, which at first sight seem to lie far apart. For example, imaginary straight lines of zero length are applied to show that the angle at the centre of a circle is twice the angle at the circumference. The style, both of wording and arrangement, is all one expects of the illustrious author, and the book is a joy and a refreshment as well as an education. H. P. H.

THE LIBRARY.

Change of Address.

The Library is now at 9 Brunswick Square, W.C., the new premises of the Teachers' Guild.

The Librarian will gladly receive and acknowledge in the *Gazette* any donation of ancient or modern works on mathematical subjects.

Scarce Back Numbers.

Reserves are kept of A.I.G.T. Reports and Gazettes, and, from time to time, orders come for sets of these. We are now unable to fulfil such orders for want of certain back numbers, which the Librarian will be glad to buy from any member who can spare them, or to exchange other back numbers for them :

Gazette No. 8 (very important).
A.I.G.T. Report No. 11 (very important).
A.I.G.T. Reports, Nos. 10, 12.